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At the 31st meeting the death of Professor A. M. Collett was announced, and Mrs. Cornelia S. Miles, first vice-president, became acting president. Mrs. Miles is principal of the Broadway School, Denver, Colorado, and has received the degree of A.M. in the graduate school of the University of Denver, and last summer was engaged in scientific work in the graduate school of the University of Chicago.

Professor George L. Cannon, who for a number of years had been engaged with Professor Collett in scientific work in the East Denver High School, gave a sketch of his life, and offered resolutions which were adopted.

Mr. E. B. Sterling delivered a lecture on 'puff balls,' obtained in Denver and vicinity, explaining the difference between them and the eastern forms. He pronounced the several species at Denver, so far as tested by his observations and experience, to be edible. His lecture was supplemented by a short address by Professor Ellsworth Bethel, a recognized authority on botany in Colorado. Professor George L. Cannon followed with an address on the 'Death of the Leaves,' contrasting the fall colors of this region with those of the East.

At the 32d meeting, 'Navajo Blankets, their History and Symbolism,' was the topic for discussion. After some introductory remarks by Dr. J. B. Kinley, Colonel U. S. Hollister spoke at length on the subject, illustrating his remarks by about sixty-five blankets from his own private collection. He described their system of weaving, use of dyes, and the meaning of the symbols.

Dr. A. L. Bennett delivered a lecture at the 33d meeting on the 'Value of the Cranial Capacity as Indicating the Degree of Intelligence Enjoyed by the Prehistoric Cliff Dwellers of our Great Southwest.' Dr. Bennett, in addition to being chairman of the Section of Anthropology and Ethnology of the Colorado Academy of Science, is also a fellow of the Anthropological Institute of Great Britain and Ireland. Dr. Bennett has spent considerable time examining and measuring the cranial capacity of the large collection of the Cliff Dweller skulls from the

Mancos region, Colorado, in the museum of the State Historical and Natural History Society of Colorado. From data obtained in these measurements he gives them a higher grade of intelligence than has been accorded by some to these primitive people.

Mrs. W. S. Peabody read a paper on the 'Work and Plans of the Cliff Dwellings Association,' being an interesting review of efforts made to preserve from vandalism and the relic hunter the prehistoric ruins of the Southwest.

WILL. C. FERRIL,
Secretary.

DISCUSSION AND CORRESPONDENCE.

THE PUBLICATION OF REJECTED NAMES.

I WISH to speak quite respectfully of Mr. T. D. A. Cockerell; but surely systematists would be much happier if he and his like did not raise such disturbing questions as that in SCIENCE for January 30, p. 189. Had he chosen to condemn Messrs. Banks and Knowlton, first for wasting time, ink and paper over names that they never intended to use, secondly for presumption in substituting their own inventions for those of Marx and Lesquereux, then one would have applauded him. But all he objects to in them is that they inadvertently happened to print the so-called MS. names a page or so ahead of the new names proposed by themselves. Mr. Cockerell does not attempt to prove that the MS. names were published five minutes earlier, and it is clear that the publication of the old and new names was simultaneous in each paper. 'The precise number of pages, lines, or words that intervened can make no difference. Suppose that Mr. Banks had written as follows: "For this species of *Filistrata* there is a choice of two names: *F. oceanea* and *F. fasciata*. The name *F. oceanea* has been found on an unpublished label, but since in my opinion it is inappropriate, I shall call the species *F. fasciata*." Now to be consistent, Mr. Cockerell would have to insist that in writing thus Mr. Banks contravened the rules of nomenclature, because he introduced *oceanea* first. 'An two men ride of a horse, one must ride behind.' Surely an author does not lose his freedom of choice before he

has finished posing the question? On the contrary, I regard the names *F. oceanea* and *F. fasciata* as equal in their pretensions, until the choice is made. Once made, the person that attempts to upset it is the true begetter of confusion.

But does Mr. Cockerell's conclusion follow from his premises? The conception underlying his application of the law of priority is that place is to be reckoned as time. Now a specific name has no standing until a description of the species denoted thereby has been published, and until the name in question has been associated therewith. Till then it is a *nomen nudum*. The name *Filistrata oceanea* is, we are told, a *nomen nudum*. Even had it been published in a previous paper, it would, in the absence of a description, have remained a *nomen nudum*. It appears first on page 50 of Mr. Banks' paper, but without description; and it remains without description for five whole pages. During all this space, it remains a *nomen nudum*. Mr. Banks may asseverate as often as he pleases that *F. oceanea* is identical with *F. fasciata*. But *F. fasciata* does not exist (for Mr. Cockerell), except as a *nomen nudum*, till page 55 is reached. Here is a description at last; but the name associated with that description is not *F. oceanea* but *F. fasciata*. It is this latter then that ceases first to be a *nomen nudum*.

The case of *Cucumites lesquereuxii* Knowlton is different; but even this may, on Mr. Cockerell's principles, be defended. For it follows from the axiom 'place = time' that every name is a *nomen nudum* until the diagnosis or description is complete. But the description of the fruit under discussion once finished, Mr. Knowlton calls it, not *Cucumites globulosus*, but *C. lesquereuxii*.

Mr. Cockerell may retort that this is mere hair-splitting and childish chop-logic. It is. But it is the natural outcome of an attempt to subject mere modes of expression to a rule obviously intended to apply to essential matters and not to the niceties of style.

To save all misunderstanding, let me repeat emphatically that I am not defending either Mr. Banks or Mr. Knowlton. I have no

sympathy with people who print names for the mere sake of rejecting them, or who tell us what they might have done or what somebody else might do if circumstances had been different, and so forth. If such action be in any degree checked by Mr. Cockerell's arguments, their publication will have had one good result.

F. A. BATHER.

MOTION OF TRANSLATION OF A GAS IN A VACUUM.
(REPLY TO MR. R. W. WOOD.)

IN the hope that if I bring around Mr. R. W. Wood to my view of the energy required to set a gas in motion of translation in a vacuum, he will not find my explanation of the energy changes which take place when a gas expands into a vacuum unnecessary, I will only take up here that view.

Mr. Wood in his second note (SCIENCE for December 5) on a communication of mine to the American Association says:

We sometimes find the statement in text-books that a gas expanding under such conditions that no work is done experiences no cooling, for example, when expanding into an infinite vacuum. It appears questionable, however, whether a gas can expand without doing work. Leaving out of consideration the internal work, *i. e.*, the overcoming of the forces of cohesion, we still have the gas in the receiver doing work in giving a motion of translation to the mass of gas thrown out into the vacuum.

I think, however, that it can be proved that no work is necessary to set a gas in motion of translation in a vacuum by the following reasoning. Suppose that in a body of gas all the molecules move with the same velocity instead of having, as we assume according to the kinetic theory, velocities varying greatly in magnitude, and that the identical velocity of all the molecules plays in other respects the same part which we attribute to the mean molecular velocity, *e. g.*, that to each degree of temperature of a gas a fixed velocity corresponds, etc. Let that gas be compressed in a receiver and then allowed to enter a vacuous vessel which communicates with the latter. What will happen? To my mind, it can hardly be conceived that anything else could take place than the uniform distribution of the